istry, to succeed Professor Frank Fanning Jewett, who retires on the Carnegie Foundation after thirty-two years of service. Dr. Menzies is an alumnus of the University of Edinburgh and has been a graduate student in Leipzig, Aberdeen and in the University of Chicago. Among European appointments Dr. Menzies was assistant professor of chemistry in Heriot-Watt College, Edinburgh, in 1898–1901, and professor of chemistry in St. Mungo College, Glasgow, from 1902 to 1908. He was research fellow in the Davy-Faraday Laboratory, London, in 1901. He is a member of the American Chemical Society, the London Chemical Society and fellow of the Royal Society of Edinburgh. Although retired, Professor Jewett will have a laboratory room in the chemistry building, and plans to give the college service in some much needed work on its mineralogical collections.

EDITH M. TWISS, Ph.D. (Chicago), has been appointed head of the department of botany, Washburn College, to succeed Dr. Ira D. Cardiff. James P. Poole, B.G. (University of Maine), has been appointed instructor in the department.

DR. HARRY BEAL TORREY, formerly associate professor of zoology in the University of California, has assumed the duties of professor of biology in Reed College, Portland, Oregon.

A. B. MCDANIEL, of the University of South Dakota, has been appointed assistant professor of civil engineering at the University of Illinois.

DISCUSSION AND CORRESPONDENCE

DRIESCH'S VITALISM AND EXPERIMENTAL INDETERMINISM

IN SCIENCE of June 16, 1911, I tried to point out the relation of perhaps the most widely known and most influential brand of vitalism —that of Driesch—to experimentation. I set forth that Driesch's vitalism results in "experimental indeterminism," such that "you can not make a statement *which will hold,* that a given arrangement of physical components will act in a certain definite way (even after you have observed how it acts)," because with the same physical configuration different entelechies, or the same entelechy in different manifestations, may be at work, determining diverse results in different cases. Thus I held that it nullifies the fundamental postulate of experimental work, that "when two cases differ in any respect there will always be found a preceding difference to which the present difference is (experimentally) due." I tried to show what a radical difference this would make between biology and other parts of science, in respect to the theory and practise of scientific work, holding it equivalent to an "admission that the principle on which experimental investigation is based breaks down when applied to biology."

In a following number of SCIENCE (July 21, 1911) Lovejoy takes sharp issue with my exposition of Driesch's vitalism, saying:

> A closer scrutiny of the doctrine's implications will, I think, disclose in it no such anarchical propensities (p. 78). I think Jennings misconceives Driesch's position in ascribing to him a wholesale "experimental indeterminism" (p. 78).

And after an exposition of Driesch's argument as he conceives it:

> There need in this be nothing arbitrary, nothing to baffle the purposes of the experimenter (p. 78). In all this argument for the non-mechanical nature of organic phenomena there is nothing whatever that necessarily "exempts from experimental determinism . . . that immense field of developmental processes which lies between the egg and the adult," or that necessarily nullifies the experimentalist's postulate that "when two cases differ in any respect there will always be found a preceding difference to which the present difference is (experimentally) due" (p. 80).[1]

And in the classifications of the kinds of vitalism given by Lovejoy in earlier papers (SCIENCE, November 26, 1909; and April 21, 1911), he does not so much as mention as one of the possible kinds a vitalism which distinguishes the organic from the inorganic in

[1] What Lovejoy gives here is in reality an exposition of the conclusions which he himself might draw from Driesch's data—assuming these to be the conclusions which Driesch draws.

this profound way. Thus if his point of view is accepted my paper quite lacks a *raison d'être;* I was combating windmills.

In my former paper I made no attempt to show that Driesch's views were of the character that I set forth, because it seemed to me (and still seems to me) that he had stated, in his published works as fully and unequivocally as it is possible in words, that they are of that character; and that, moreover, his whole argument loses its coherence and becomes incomprehensible if they are not.[2] I therefore did not expect any one who had made a careful examination of Driesch's "Science and Philosophy of the Organism" to question this.

Since, however, it has been questioned by one so competent as Lovejoy, with the intimation, as quoted above, that my own scrutiny had not been sufficiently close, it is of interest to learn Driesch's own opinion on this point, when the matter at issue is put explicitly before him. I quote, by permission, from letters received from Dr. Driesch:

You are quite right in saying "the biologist can not from a knowledge of the total physical configuration predict what will happen even after he has observed it." This is indeed a consequence of my vitalism and I am very glad to see that you fully appreciate it.

I reject absolute indeterminism but *accept* experimental indeterminism.

In other words: A complete knowledge of all physico-chemical things and relations (including possible relations) of a given system at the time t gives *not* a complete characteristic of that system in the case that it is a living system.

[2] Driesch's argument is one by exclusion, running essentially as follows: Since there are no diversities in the physical conditions that explain satisfactorily the diverse results in certain different cases, and since we must hold to determinism, it follows that there must be something non-physical (*i. e.,* entelechy) to account for the diversities in results. It appears to me that the failure to correctly apprehend Driesch's argument is what causes Lovejoy to intimate frequently that the entelechy concept is superfluous in Driesch's vitalism; merely "dragged into the situation," as he expresses it. Without entelechy a yawning hiatus is left in Driesch's system; it is all that saves him from absolute indeterminism.

Or: Two systems, absolutely identical in every physico-chemical respect, may behave differently under absolutely identical conditions, in case that the systems are living systems.

For: the specificity of a certain entelechy is among the complete characteristics of a living organism, and about this entelechy knowledge of physico-chemical things and relations teaches *nothing.*

My short formula about the matter in question is: No absolute, but "experimental" indeterminism.

Dr. Driesch's statements of the matter are then fully as strong as my own. If he understands his own philosophy, it therefore appears to me that the further reasoning in my former paper was quite justified, and is entitled to the careful consideration of any others who have leaned toward Driesch's vitalism without realizing that it means experimental indeterminism.

H. S. JENNINGS

ZOOLOGICAL NOMENCLATURE

TO THE EDITOR OF SCIENCE: In SCIENCE for August 9, my esteemed friend Dr. Kingsley, makes a plea for various exceptions to the rule of priority in names of animals and to other rules which have been adopted by the Commission on Nomenclature of the International Zoological Congress.

It is no doubt exasperating to many zoologists who have to use only a few systematic names in their work and then at long intervals, to find that in these intervals older names, carelessly or ignorantly neglected in the past, have risen to take their places. It is also exasperating to professional taxonomists and students of geographic and other relations of species, to be told that their efforts to bring past confusion into order shall be set aside whenever these efforts discommode workers in other fields of zoology, who for the most part neither know nor care for the part accurate bookkeeping must play in the study of systematic zoology and botany.

Taxonomy with geographical and geological distribution constitutes a science by itself,